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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Synchrony of trend shifts in Sahel summer rainfall and global oceanic evaporation, 1950–2012" *by* A. Diawara et al.

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Overview: Using reanalysis and observation-based datasets, authors examined the summer precipitation changes defined as the trend difference between 1950-1984 and 1985-2012 periods. In Sahel region, precipitation shows a drying trend for 1950-1984 but a wetting trend for 1985-2012. The similar trend shift is also found in latent heat flux anomalies averaged over the global ocean. Authors also pointed out that this trend shift in latent heat flux anomalies is consistent with a SST trend shift in the northern hemisphere and a wind speed shift in the southern hemisphere.

This result may have an important implication for global hydrological cycles that may also influence the Sahel rainfall variability. The good point in this manuscript is that

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authors clearly described what they did. However, I feel that this manuscript have to present their findings more logically. Although I can understand "what" they did, I cannot understand "why" they did it. Why does this manuscript examine the trend shifts in Sahel summer rainfall? Why did author take the Sahel region illustrated by the black box in Fig 1 and the period for 1950-1984 and 1985-2012? Why do we need to focus on the evaporation averaged over the "global" ocean instead of the "regional"? I would like to encourage them to revise this manuscript with more logical way instead of a description. The following is more detailed point that might be helpful to revise this manuscript.

Reply: Thank you for your advice. We will address the points in the revised paper accordingly.

1. First, I cannot understand what is the main goal of this manuscript and what is the scientific question that authors want to clarify in this manuscript. In introduction, authors pointed out the local and remote SST effects on decadal rainfall variability over Sahel region. Based on these previous studies, authors described the current problem of previous studies: "the exact linkage between the multi-decadal variation of Sahel rainfall and the global ocean is unclear" on Line 10 Page 11271. So we expect that this manuscript will reveal the exact linkage about multi-decadal rainfall variability. However, this manuscript examined the "trend shift" instead of multi-decadal variability. I confused why this manuscript focused on the trend shift and why did they choose trends for those specific periods.

Reply: Your comment is why we have focused on looking at changes in Sahel rainfall that occurred in about 1985? Our approach is constrained by data availability and data quality. We are unable to study multi-decadal variability directly because there was only sufficient data to look at 1 or at most 2 cycles, so we focused on a phenomenon ("the trend shift") that might indicate a change in phase in multi-decadal variability. But, we didn't describe the reason why in 1985 in the previous version. Therefore, we will address this explanation in the revised manuscript in particular in the introduction

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section.

2. Second, I have no idea why they assumed that "moisture transport from different parts of the world ocean may have some effects on precipitation over Africa", as described on Lines 13-15 Page 11271. According to the moisture balance equation, the precipitation anomalies balance the moisture divergence and evaporation anomalies. This equation is based on the local process and I hesitate to assume that the evaporation variability in the Pacific affects to the Sahel rainfall. The tropical Pacific SST variability could induce changes in atmospheric circulation, which may influence the moisture flux divergence over Sahel region. So I can agree that the global SST variability plays some roles for Sahel rainfall variability. However, it is not make sense for me that the evaporation in the tropical Pacific affects Sahel rainfall variability via moisture transport. Finally, there is no scientific evidence to explain the possible physical linkage as summarized in schematic diagram in Fig 8. According to this manuscript, the SST trend shift induces the trend shift of latent heat flux in the northern hemisphere whereas the wind speed trend shift contributes to trend shifts of latent heat flux and SST in the southern hemisphere. In other words, this manuscript assumes that changes in all of those variables are in phase. However, the latent heat flux anomalies (and wind speed anomalies) contribute to "tendency" of SST anomalies, which means that the SST anomaly change would be out of phase compared to changes in wind speed or latent heat flux anomalies. More logical explanation for this manuscript would be needed.

Reply: The summary of the comment is why we assume a connection between global evaporation and local rainfall? We should expand our discussion of the literature cited in this sentence: "Sahel rainfall is known to be related to nearby SST (Lough, 1986; Bader and Latif, 2003; Chung and Ramanathan, 2006) and remote SST (Folland et al., 1986; Janicot et al., 1996; Rowell, 2003; Fontaine et al., 2011; Munemoto and Tachibana, 2012; Diatta and Fink, 2014)." The fact that the Sahel rainfall is correlated with "remote SST" suggests that global-scale processes are potentially important, and

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it is not just an assumption that we have made. However, we didn't describe this information by the previous studies in detail. We will address the explanation on why we focus on global evaporation and local rainfall in the revised version. A global map of the horizontal moisture flux that is correlated with the Sahel rainfall will be added to show how remote oceanic evaporation influences the Sahel rainfall. Thank you.

3. Finally, there is no scientific evidence to explain the possible physical linkage as summarized in schematic diagram in Fig 8. According to this manuscript, the SST trend shift induces the trend shift of latent heat flux in the northern hemisphere whereas the wind speed trend shift contributes to trend shifts of latent heat flux and SST in the southern hemisphere. In other words, this manuscript assumes that changes in all of those variables are in phase. However, the latent heat flux anomalies (and wind speed anomalies) contribute to "tendency" of SST anomalies, which means that the SST anomaly change would be out of phase compared to changes in wind speed or latent heat flux anomalies. More logical explanation for this manuscript would be needed.

Reply: Your next comment is that what is the evidence for the schematic in Fig. 8? SST response shouldn't be in phase with Latent Heat Flux. There should be a delay between the positive latent heat flux anomaly and the negative SST anomaly..Thank you for good advice. It's possible that there are timescale effects to be considered. If the lag between the latent heat flux anomaly and the SST response is a few months, the trend shifts are still relatively in phase. For the purpose of understanding the multi-decadal time scale like the trend shift, the time lag between the two is negligibly small. However, we did not write this possibility of lag. So, we will add that this monthly-scale lag effect is small in the revised version with showing lag-correlation. In addition, yearly-scale lag-correlation map will be shown in the revised version.

4. Didn't Zhang and Delworth (2006) demonstrate that Sahel rainfall is strongly correlated with the Atlantic Multidecadal Oscillation? In that case, the trend shift could be explained by whatever is responsible for the AMO. That is the missing teleconnection. What does this study tell us that we do not learn from Zhang and Delworth (2006)?

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Reply: Thank you for your suggestion on the AMO. As in your comment, Sahel rainfall and the AMO do seem strongly related. Zhang et al. 2006 focused on valiging the SST anomalies averaged over the entire North Atlantic while the design of ACE simulations does not allow them to modify the oceanic heating outside of the Atlantic which means that their model did not integer a possible contrast between NS-SST. In our study, we based our research more likely on that Munemote et al 2012. Our results have shown consistent results of visible interaction between NS-SST through an global evaporation processes. But, in the previous version, we did not describe the relation to the AMO as in Zhang et al. 2006. We should have referred this important paper and should have address that how our results differ from Zhang et al. 2006. We will address this in the revised version.

5. Could you please show (not merely say) that the differences in the reanalysis datasets do not matter for this analysis?

Reply: As shown in the text, the dependence in the choice of the dataset is small. In the revised version, we will summarize that dependence on the choice of used dataset by showing in additional table or figures. Thank you.

Minor concerns 1. Lines 22-23 Page 11270 "Many studies have shown that rainfall varies greatly in the Sahel": This sentence is unclear. Does Sahel have the largest precipitation variance over land? What timescale do you want to say?

Reply: Sahel might probably not have the largest precipitation variance over land however the tendency of it precipitation times series have shown a tremendous change over the last sixty years non negligeable,please see Figure 2. This fact had been reported by a several research papers. We pointed out the behavior of the tendency and took advantage of the picks in the trend shift factor. This information will be addressed in the revised version.

2. Line 9 Page 11271 "long-term climate trends are generally related to the state of the ocean": This sentence is unclear. We may say that a long-term temperature trend is

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generally related to atmospheric CO2 increase. But I don't know what kind of long-term climate trends authors want to say.

Reply: Along with the CO2 influence, ocean interior process also has the time scale with multi-decadal. We should have said this. In the revised version, we will address this.

3. Lines 13-15 Page 11271 "It is reasonable that ..." As I described in my major comment,this is not reasonable for me.

Reply: We considered all part of the oceans map and confirmed some previous studies as Omotosho et al 2008 focusing on the change in ITCZ position or the track, another example is Giavanni et 2003 which investigates the global oceanic warming over Africa. Considering the fact that the oceanic moisture flux has an important role to play into Africa and precisely over Sahel region could explain some lacks of understanding the actual phenomenon in the region. We will address this in the revised version.

4. Line 18 Page 11272 "defined as the region ..." Why do authors define this region? It may be good idea to show the standard deviation or variance for precipitation.

Reply: Thank you so much. In this study, our first target has been to show the inside influence of oceans over Sahel region, taking between latitude 10-20N allowed us to remove direct influence of coast deep to 100 km front line. We will describe the reason in the revised version along with standard deviation.

5. Line 10 Page 11274 "SST decreased until 1984" From the spatial map of SST trends difference, I cannot see the SST decreasing until 1984.

Reply: SST has a changing phase from 1984, using the SST over Northern and Southern we could not see visible evidence of change, however the difference between them has shown a contrast. So the Northern hemisphere SST became a lower decreasing than the Southern hemisphere SST and described an opposite trend after 1984. This kind of description will be added in the revised version. Thank you.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 11269, 2015.